

Employment Subsidies, Firm Capital Formation and Skill Intensity

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EMPLOYMENT SUBSIDIES, FIRM CAPITAL FORMATION AND SKILL INTENSITY

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Abstract

This study investigates the impact of employment subsidies on the dynamics of labor markets and production technologies in Turkey's manufacturing sector, with a focus on a 2016 subsidy reform that extended a 6-percentage-point reduction in employer-side social security contributions to previously ineligible micro firms. Relying on a difference-in-differences approach, we examine the effects of the policy on employment, capital investment, capital intensity and skill upgrading across firms of varying sizes. Our findings reveal that micro firms responded with significant employment gains, while small firms increased capital investment and capital–labor intensity in the form of tangible assets. However, the policy had no measurable effect on skill upgrading, highlighting its limits in fostering structural labor market transformation. By extending the evaluation of employment subsidies beyond job creation, this study contributes to the broader discourse on active labor market policies and their role in promoting sustainable growth and technological development in emerging economies.

Keywords: Employment subsidy, capital intensity, firm-level analysis, difference-in-differences

JEL Classifications: J23, J38, R58

ملخص

تدرس هذه الدراسة تأثير إعانات التوظيف على ديناميكيات أسواق العمل وتقنيات الإنتاج في قطاع التصنيع في تركيا، مع التركيز على إصلاح الدعم في عام 2016 والذي أدى إلى خفض مساهمات الضمان الاجتماعي من جانب صاحب العمل بنسبة 6 نقاط مئوية للشركات الصغيرة غير المؤهلة سابقًا. ومن خلال الاستفادة من نهج الاختلاف في الاختلافات، نقوم بدراسة آثار السياسة على التوظيف واستثمار رأس المال وكثافة رأس المال ورفع مستوى المهارات عبر الشركات ذات الأحجام المختلفة. تكشف نتائجنا أن الشركات الصغيرة استجابت بمكاسب كبيرة في التوظيف، في حين زادت الشركات الصغيرة من الاستثمار الرأسمالي وكثافة رأس المال – العمالة في شكل أصول ملموسة. ومع ذلك، لم يكن لهذه السياسة أي تأثير ملموس على رفع مستوى المهارات، مما يسلط الضوء على حدودها في تعزيز التحول الهيكلي لسوق العمل. ومن خلال توسيع نطاق تقييم إعانات التوظيف إلى ما هو أبعد من خلق فرص العمل، تساهم هذه الدراسة في الخطاب الأوسع حول سياسات سوق العمل النشطة ودورها في تعزيز النمو المستدام والتنمية التكنولوجية في الاقتصادات الناشئة.

1. Introduction

Active labor market policies (ALMPs) are designed to address structural labor market challenges such as rising inequality, technological disruption, and the changing nature of work. These policies include job guarantees, universal basic income, work-sharing arrangements, retraining and reskilling programs, employment subsidies, and cooperative business models. The main goal is to either complement or serve as alternatives to traditional mechanisms such as minimum wage laws, collective bargaining, and unemployment insurance. By promoting both labor market flexibility and security, ALMPs seek to enhance economic resilience, reduce inequality, and foster more inclusive labor markets that better serve the needs of both workers and employers.

Among the most widely adopted ALMPs are employment (or wage) subsidies, which provide financial incentives to employers with the goal of increasing employment, particularly among disadvantaged groups such as long-term unemployed individuals, youth, low-skilled workers, and marginalized populations. These subsidies can take various forms, including direct wage payments, tax credits, or reductions in employer-side social security contributions. They are often implemented during periods of economic downturn or recession, when labor demand weakens and unemployment risks intensify. The main rationale behind employment subsidies is to lower the cost of employment, support vulnerable populations by increased household income and consumption, alleviate poverty, particularly for low-wage workers and encourage formalization, especially in countries with high levels of informal employment.

However, employment subsidies also face several limitations. A central concern is deadweight loss: many subsidized jobs may have been created even without the subsidy, thereby reducing the cost-effectiveness of the intervention. Additionally, substitution effects may arise if firms replace unsubsidized workers with subsidized ones, particularly when the subsidy is temporary. There is also the risk of dependency, whereby firms refrain from hiring unsubsidized workers once the incentive is withdrawn, potentially undermining the sustainability of job creation. Finally, employment subsidies often entail substantial fiscal costs, which can be especially burdensome if the policy is broad-based or extended over long durations.

Employment subsidies have long been a component of Turkey's ALMP toolkit, implemented in various forms. These programs have targeted a wide range of objectives, including first-time employment, specific economic sectors, population subgroups such as women or youth, particular firm characteristics (e.g., small businesses or exporters), or specific geographic regions such as provinces or counties. Regardless of the specific target group, the primary design of these policies has consistently emphasized reducing the labor cost burden for employers, rather than providing direct income support to employees (Aşık et al., 2022).

This emphasis on cost reduction is particularly relevant in Turkey, where the combined burden of taxes and social security contributions on labor is 39% of gross labor cost, above the OECD average of 35% (OECD, 2025). Consequently, from the perspective of firms, any meaningful

reduction in labor costs through subsidies can affect a wide array of operational and strategic decisions. Lower marginal costs raise labor demand, which in turn shapes hiring behavior, production capacity as increased hiring supports output expansion, and pricing strategies. They may also influence human capital choices affecting the skill composition of the workforce and capital accumulation and technological upgrading through shifts in factor substitution or investment incentives.

The expected outcomes for human capital formation and capital investment are less clear-cut and may even be interrelated, both with important implications for firm-level productivity. On the human capital side, evidence suggests that employment subsidies, particularly those targeting low-skilled labor, may increase the share of low-skilled employment and potentially dampen incentives for skill acquisition (Oskamp and Snower, 2006). However, from a theoretical standpoint, it is equally plausible that firms could leverage the cost advantage created by the subsidy to hire higher-skilled workers at a reduced effective wage, thereby upgrading the skill composition of their workforce. As such, the impact of employment subsidies on workforce skills is ambiguous and may vary across sectors, firm types, and policy designs.

On the capital side, the effects of lower labor costs are similarly dual. Firms may respond by expanding their workforce as a substitute for capital, especially if labor becomes relatively cheaper in production. Alternatively, some firms might increase capital investment, either to complement a growing workforce and maintain the marginal productivity of labor, or by reinvesting the cost savings derived from the subsidy into technological upgrading or capacity expansion. Classical and neoclassical frameworks generally support the complementarity of production factors, predicting that labor subsidies could stimulate capital accumulation (Judd, 1987; Shi and Wen, 1999; Fuest and Huber, 2000; Daveri and Tabellini, 2000). In contrast, substitution effects are also possible. Petrucci and Phelps (2005), for example, argue that subsidies tend to disproportionately benefit the targeted factor, potentially crowding out investment in the untargeted one, suggesting a trade-off rather than complementarity in some settings.

In the existing literature, studies evaluating the effectiveness of employment subsidies have primarily focused on their impact on the level of employment, with the bulk of this research concentrated in developed countries (Blundell et al., 2004; Deidda et al., 2015; Goos and Konings, 2007; Huttunen et al., 2013). The findings are mixed. Some studies report no statistically significant effects, while others find positive employment impacts for specific groups, such as older women and youth, or observe gains that are transitory in nature. Evidence from developing countries is relatively more limited but similarly heterogeneous: some studies report negligible or null effects overall, while others identify positive impacts on formal employment, particularly in high-informality contexts (Gruber, 1997; Kugler and Kugler, 2009; Cruces et al., 2010).³

³ See Almeida et al. (2014) for a review for developing economies.

Turkey has also been the subject of several empirical evaluations of employment subsidy programs. For example, Betcherman et al. (2010) document a positive effect on formal employment from two regional subsidy programs launched in 2004 and 2005, which targeted new hires in firms with more than 10 employees. The 2011 employment subsidy, targeted at young people and women has been evaluated using the Household Labor Force Survey in studies by Ayhan (2013), Uysal (2013), and Balkan et al. (2016). While Balkan et al. (2016) find no measurable effect, the other two report positive outcomes, especially for women, though concerns remain about the duration and sustainability of these effects. More recently, Basbuga et al. (2022) assess the impacts of multiple subsidy schemes and find positive employment outcomes. The most comprehensive analysis to date is by Aşık et al. (2022), who evaluate a geographically targeted 2016 subsidy that reduced employer social security contributions in underdeveloped regions. Using firm-level administrative data from the Social Security Institution, the study finds a significant and persistent increase in registered employment, particularly in small firms. However, they argue that the effect primarily reflects formalization of pre-existing informal jobs, rather than expansion at the extensive margin.

While these studies provide valuable evidence on employment outcomes, limiting policy evaluation solely to employment levels risks overlooking broader firm responses. In particular, the impact of employment subsidies on workforce skill composition and firm capital investment behavior has critical implications for productivity growth and long-term competitiveness. These dimensions are especially important in middle-income economies like Turkey, where productivity stagnation and low investment rates constrain inclusive development.

Against this background, we extend prior analyses by examining the broader firm-level effects of employment subsidies. Specifically, we investigate the causal impact of the 2016 regional subsidy expansion on capital accumulation and skill composition in the manufacturing sector, using high-quality matched employer–employee administrative data. In doing so, we provide a deeper understanding of how wage subsidies affect both labor and capital dynamics in developing economies. We restrict our analysis to manufacturing industry only as the main focus of this research is to investigate the relationship between employment subsidies and productive capital formation.

To this end, we examine the effects of a geographically targeted employment subsidy introduced under Law No. 6846, which provided an additional 6-percentage-point reduction in employer-side social security contributions.⁴ This subsidy was applied on top of the existing, nationwide 5-percentage-point reduction under Law No. 5510 and was specifically designed to encourage registered employment in underdeveloped regions⁵ of Turkey.

⁴ This is the same subsidy program Aşık et. al (2022) investigates

⁵ Turkey's regional development framework classifies provinces into six development regions based on socioeconomic indicators. Region 1 represents the most developed areas, while Region 6 includes the least developed. Regions 4, 5, and 6 are officially designated as underdeveloped and are thus eligible for targeted policy intervention.

The additional 6-percentage-point reduction in employer social security contributions was first introduced in 2013 under Law No. 6486, as an added incentive to support firms operating in underdeveloped areas. A key institutional feature of the 2013 subsidy, which is central to this study's identification strategy, was its eligibility restriction: Initially, the subsidy applied only to firms with 10 or more employees, and even then, only to new hires beyond a firm's existing workforce. This changed markedly in 2016, when the subsidy's scope was significantly broadened. From March 2016 onward, all firms operating in underdeveloped regions became eligible, regardless of their employment size, and the subsidy was extended to all employees, not just additional hires. This reform marked a turning point for micro firms, defined as those with fewer than 10 employees, which had previously been excluded from the program and now became eligible for the first time. However, since the 2016 reform applied to all workers of a firm, the subsidy likely affected labor costs of not just the micro firms but also the small, medium and large firms as well.

A second major reform occurred in August 2020, when the spatial unit used to define subsidy eligibility shifted from the province level to the county level. Under this new classification, several counties which were previously assigned as Regions 1, 2, or 3 due to their provincial affiliation, were reclassified into Region 4, 5 or 6 based on their own local socio-economic development indicators. This change not only expanded the program's reach but also created a natural comparison group for empirical analysis, as these counties remained untreated during the earlier subsidy expansion analyzed in this study.

We leverage this second legislative change, the 2020 reclassification of subsidy eligibility based on counties rather than provinces to construct a refined control group for identifying the causal effects of employment subsidies on firms in treated areas between 2006 and 2019. We implement a difference-in-differences (DiD) framework to estimate the causal impact of the 2016 subsidy expansion, exploiting variation in eligibility criteria over time. Similar to Aşık et al. (2022), we treat the 2016 reform as the key policy intervention and compare firm outcomes for those located in Region 4 counties, with firms that were classified under Regions 1, 2, or 3 until 2020 but reclassified as Region 4 counties after the legislative change in 2020, which is after our sample period.⁶

More specifically, to construct a valid control group, we draw on the reclassified counties as subsidy-eligible only after 2020, despite having socioeconomic development levels comparable to Region 4 but were not treated during the sample period of 2006 to 2019 due to their provincial affiliation with more developed regions (Regions 1–3). This change in policy design, shifting the eligibility unit from provinces to counties in 2020 created a quasi-natural experiment, enabling us to compare treated firms with otherwise similar untreated firms that remained outside the program until after the study period. By restricting our analysis to 2006–2019, we ensure that control units are not contaminated by later eligibility. We also restrict our sample to

⁶ The key reason why Aşık et al. (2022) and our study focus on Region 4 counties is because after the legislative change in 2020, there were only a few counties that were downgraded from Regions 1, 2 and 3 to Region 5 or 6. Furthermore, treatment counties in Regions 5 and 6 tend to be further in the East part of Turkey which might be quite different in unobservable ways to the firms in regions 1, 2 and 3 which tend to be located in the more developed and industrialized West part of Turkey.

firms that already existed before March 2016 to shut down firm entry channel as a response to the subsidy.

Our analyses rely on exceptionally rich employer-employee matched administrative data which covers the universe of all registered firms and workers outside the finance and defense sectors in Turkey. Administrative data includes information on firms' location, sectors, and employment size. Critically, dataset includes information on firms' balance sheets, firm-to-firm trade links as well as imports and exports.

We find that the 2016 employment subsidy had statistically significant effects on micro and small manufacturing firms. As also shown by Aşık et al. (2022), micro firms in treated Region 4 counties experienced statistically significant employment growth, while small firms responded with increases in capital investment and capital intensity, particularly in tangible assets. These effects were most pronounced among firms near the 10-employee threshold targeted by the policy. In contrast, medium and large firms, despite benefiting the most in monetary terms, exhibited no measurable response in employment or capital outcomes. Additionally, the subsidy had no significant effect on the skill composition of the workforce, suggesting limited structural upgrading as well as no impact on net real profits or profits to sales ratios for any firm size type.

Following Dube, Lester and Reich (2010) and Card and Krueger (1994), we implement an additional robustness check that narrows the comparison to treated and control firms located in adjacent border counties. Although in our preferred identification strategy, the control counties which were reclassified into Region 4 in 2020 are comparable to the treated counties in measured socio-economic development levels, they lay within provinces classified in Regions 1 to 3 throughout the sample period. Therefore, they may be affected by the provincial economies exhibiting different sectoral compositions and different underlying growth paths. Our linear trends and industry-by-year fixed effects relax the strict parallel trends requirement, but they do so parametrically and cannot absorb local shocks that evolve non-linearly. The border-pair design addresses this concern directly. We restrict the sample to treatment counties adjoining a control county, pool all such pairs into a single regression, and add pair-by-year fixed effects to our baseline specification. These absorb any shock common to a given pair in a given year, including weather, movements in local input costs and fluctuations in regional demand, without imposing any functional form on how such shocks evolve. Identification therefore derives entirely from the contrast between treated and control firms observed within the same pair and the same year.

We contribute to the literature in two important ways. First, we add to the relatively limited body of research on the effects of employment subsidies in Turkey and other developing economies, where program evaluations remain scarce despite widespread policy use. Existing evidence is concentrated mostly in high-income settings, and the studies that do address middle-income contexts have focused almost exclusively on labor market outcomes.

Second, our paper is among the few to examine the relationship between employment subsidies, capital formation and the skill composition of employment. Our study departs from Aşık et al. (2022) which evaluate the same social security contribution subsidy, however their question concerns the composition of the employment response, namely whether the formal employment gains represent net job creation or the formalization of workers who were already employed informally. Ours concerns what firms do with the cost saving beyond the labor margin. A reduction in the price of labor alters relative factor prices, and standard theory gives no unambiguous prediction as to whether firms respond by substituting toward labor, by expanding scale and accumulating capital alongside it, or by upgrading the composition of the workforce. Our study aims to present empirical evidence to fill this gap. We also examine heterogeneity across the firm size distribution rather than restricting attention to a single size band. Capital responses, however, are unlikely to be uniform across size classes, since access to finance, the lumpiness of investment and the scope for factor substitution all vary with firm size. We therefore estimate effects separately for micro, small, and medium and large firms, and show that the capital response is concentrated in small sized firms.

The paper proceeds as follows: Section 2 describes the data set. Section 3 presents the empirical methodology and identification strategy. In Section 4, we share the results of the empirical analyses. Section 5 concludes.

2. Data

Our study relies on a rich administrative microdata obtained from the Entrepreneurship Information System (EIS) of the Ministry of Industry and Technology. EIS is a large-scale data integration platform that consolidates firm-level information from multiple public institutions in Turkey. EIS integrates information from several key administrative sources from i) Revenue Administration (GİB) for financial declarations, including balance sheets and income statements; ii) Social Security Institution (SGK) for employment-related data, such as firm registrations, insured employees, and demographic characteristics, and iii) Ministry of Trade for foreign trade records.

The database covers all enterprises subject to corporate and income taxes that generate income from commercial or industrial activity and employ workers registered with SGK. It excludes non-commercial entities (e.g., residential associations), freelance professionals (e.g., lawyers, architects), banks, and joint ventures governed under separate reporting rules. EIS includes over 3 million registered firms outside the finance and defense sectors. The data is available at province and county levels, enabling fine-grained geographic analysis. Most variables are consistently available from 2006 onward, allowing for longitudinal analysis of firm outcomes.

The firms can be classified across regions, counties and subsidy regions, sectors (NACE Rev. 2) and firm-size. Size classes are based on official Turkish definitions, combining employee count and annual turnover. As for the employee counts, the classification is the following: i)

Micro: 0–9 employees, ii) Small: 10–49 employees, iii) Medium: 50–249 employees and, iv) Large: 250+ employees. We restrict the analysis to firms operating in the manufacturing sector.

EIS provides firm-level employment data disaggregated by age, gender, occupation, and wage level. While individual education levels are not recorded, the dataset includes International Standard Classification of Occupations (ISCO) codes for each job title. However, ISCO codes for workers are only available starting from the year 2014. We use ISCO classifications to differentiate between high-skill and low-skill employment segments in our analysis.

We report the summary statistics for the main outcome variables in Table 1, disaggregated by firm size and by treatment status over the 2006 to 2019 sample period. All variables are in natural logarithms consistent with the baseline regressions that we report in Tables 3 to 6.⁷ Among micro firms, treated and untreated firms are closely comparable, with treated firms marginally more capital intensive on both measures of capital and marginally smaller in employment. Among small and among medium and large firms, treated firms are smaller than untreated firms on each dimension. As Table 1 shows, differences among the treated and untreated firms are statistically significant, however, given the large number of observations, significance could be of a limited diagnostic value here. The reason is that the standard error of a mean difference declines with the square root of the number of observations, and with large samples, even economically insignificant gaps might register as significant.

Therefore, following Imbens and Rubin (2015), we assess comparability using the normalized difference, reported in the final column as the difference in group means divided by the square root of the average of the two within-group variances. Because the denominator is a measure of dispersion rather than a standard error, this statistic does not increase mechanically with sample size and can be read directly as the separation between the two covariate distributions in standard deviation units. Imbens and Wooldridge (2009) suggest that as a rule of thumb, normalized differences below 0.25 in absolute value indicate overlap sufficient for linear regression adjustment to be reliable, while larger values signal sensitivity to functional form. As the last column of Table 1 shows, every normalized difference for our outcome variables falls well within this range, and the largest difference does not exceed 0.17. Furthermore, as will be explained in more detail Section 3, our identification does not require balance in levels, since the firm fixed effects in equation (1) absorb time-invariant differences between the treated and the untreated groups. Instead, the identification relies on the conditional parallel trends assumption; namely that controlling for firm fixed effects, NUTS-2 linear time trends and 4 digit industry-year fixed effects, treated and untreated firms would have followed common trajectories in the absence of the 2016 reform. We assess the plausibility of parallel trends assumption directly through the event study specifications reported in Section 4.

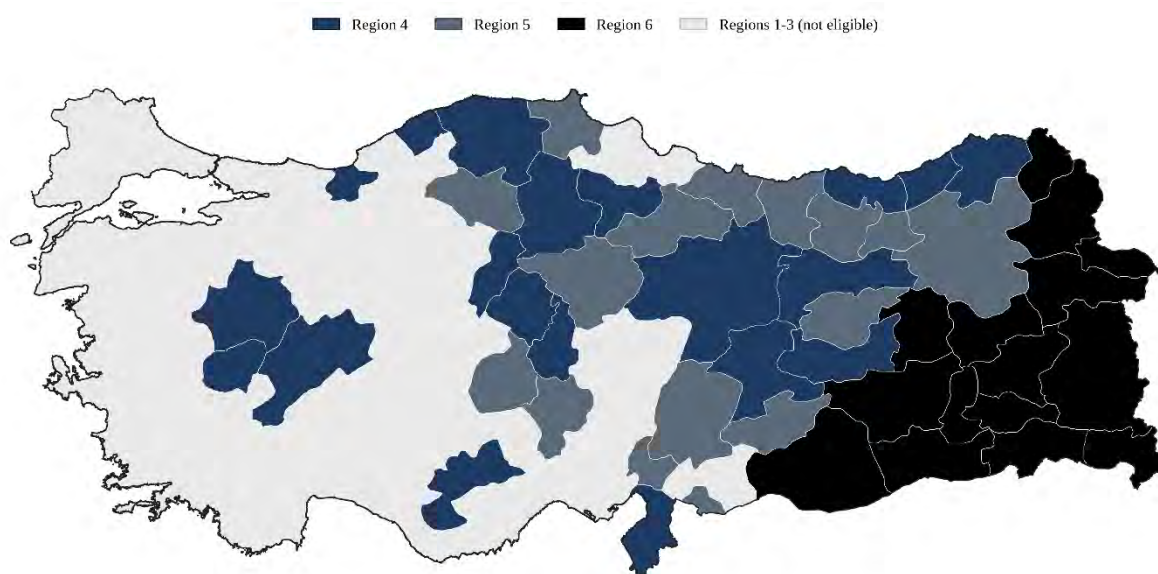
⁷ We apply a $\log(1+x)$ transformation rather than a $\log$ transformation, since the latter would drop firm-year observations with zero reported capital and select the sample on an outcome that is itself plausibly affected by the reform. Chen and Roth (2024) show that treatment effects estimated on $\log(1+x)$ depend on the units of the outcome where treatment affects the extensive margin, and cannot be read as percentage effects. We accordingly interpret our coefficients as effects on the transformed outcome rather than as proportional changes.

3. Methodology

Employer-borne social security contributions in Turkey are reduced through two schemes that operate in sequence. The first, established under Law No. 5510 and commonly referred to as the five-point reduction, lowers the employer's share of the disability, old age and death insurance premium from 11 to 6 percent. It applies nationwide and is granted automatically to any firm that satisfies its conditions, namely that it has no arrears on premiums or administrative fines, pays its insurance premiums on time, and employs no fictitious workers. In practice this makes the five-point reduction an inducement to comply with formal labor market regulation. The second scheme, introduced in May 2013 under Law No. 6486 and known as the additional six-point subsidy, is geographically targeted and eliminates the remaining 6 points of the employer's share of the same premium. Receipt of the five-point reduction is a necessary condition for the additional six points, so that firms qualifying for both are relieved entirely of the 11 percent employer contribution to disability, old age and death insurance. The two together cover close to half of employer-borne social security contributions and around 11 percent of total labor costs for a minimum wage employee, with the additional six points alone accounting for a reduction in total labor costs of roughly 6 percent. Eligibility for the additional six points requires that the firm operate in an eligible area. Once a firm in an eligible area receives the five-point reduction, the additional six points is applied automatically each month without any application.

The Turkish government classifies all provinces and counties into six development regions based on a Socio-economic Development Index (SEGE), prepared by General Directorate of Development Agencies (GDDA) of Ministry of Industry and Technology. Region 1 is the most developed and Region 6 is the least developed region. SEGE score computation is based on a principal component analysis and takes into account high number of variables (56 as of 2022) in the dimensions of demography, employment and social security, education, health, finance, competitiveness, innovation and quality of life (GDDA, 2023). The additional 6-point subsidy was targeted to Regions 4, 5, and 6, with eligibility initially defined at the provincial level (Figure 1).

Figure 1. Provinces eligible for the 6 additional points reduction in 2016



In March 2016, the Turkish government increased the coverage of the 2013 subsidy to include micro firms with less than 10 employees and cover all workers of a firm instead of just the new hires of a firm. We estimate the causal effect of this 2016 policy reform in Turkey that expanded eligibility for an additional 6-percentage-point reduction in employer-side social security contributions. The reform created a quasi-natural experiment, which we exploit using a difference-in-differences (DiD) framework, comparing changes in outcomes for treated and untreated firms before and after the reform.

Table 1. Summary statistics in logarithms, by firm size and treatment status

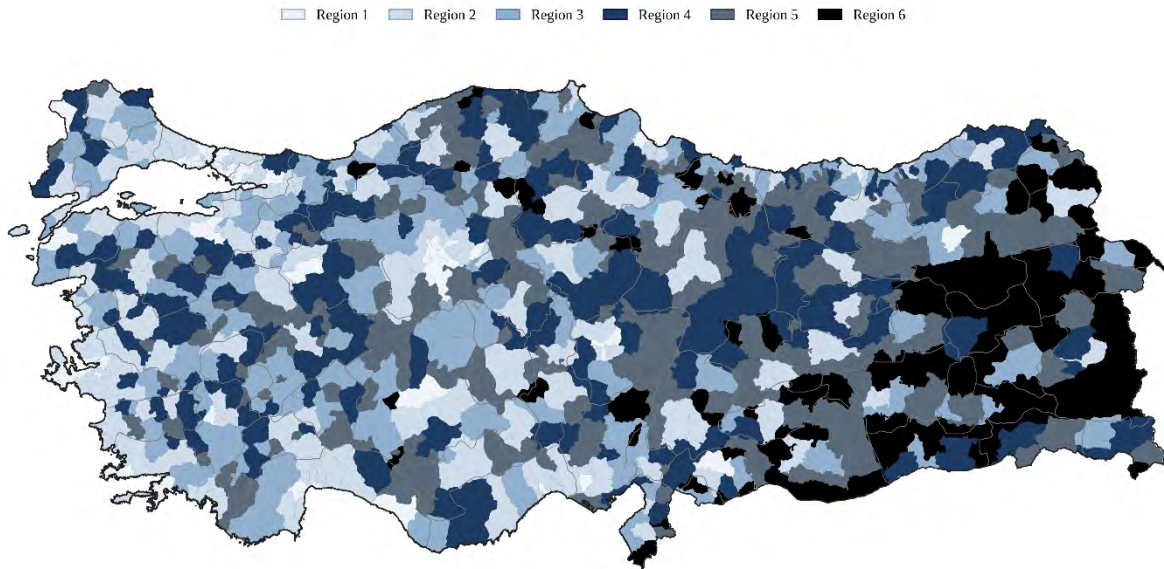
Variable	Full sample		Treated		Untreated		Difference	Norm. diff.
	Mean	SD	Mean	SD	Mean	SD		
Panel A. Micro firms								
Log employment	0.269	0.544	0.259	0.539	0.278	0.549	-0.019***	-0.035
Log machinery and equipment	0.246	1.159	0.253	1.174	0.236	1.136	0.018***	0.015
Log tangible assets	0.578	1.762	0.607	1.790	0.540	1.724	0.067***	0.038
Log capital-labor ratio (machinery and equipment)	0.783	0.869	0.794	0.889	0.770	0.841	0.024***	0.028
Log capital-labor ratio (tangible assets)	1.044	1.340	1.074	1.382	1.007	1.283	0.067***	0.050
Observations	182,797		99,271		85,771			
Panel B. Small firms								
Log employment	1.998	1.162	1.956	1.166	2.074	1.152	-0.118***	-0.102
Log machinery and equipment	3.661	3.529	3.494	3.521	3.962	3.524	-0.469***	-0.133
Log tangible assets	5.462	3.433	5.256	3.464	5.835	3.346	-0.579***	-0.170
Log capital-labor ratio (machinery and equipment)	2.518	2.383	2.434	2.385	2.670	2.373	-0.236***	-0.099
Log capital-labor ratio (tangible assets)	3.892	2.466	3.761	2.484	4.129	2.414	-0.368***	-0.150
Observations	16,366		10,540		5,826			
Panel C. Medium and large firms								
Log employment	3.660	1.605	3.585	1.582	3.779	1.635	-0.194***	-0.121
Log machinery and equipment	7.194	3.620	7.099	3.585	7.344	3.671	-0.245*	-0.067
Log tangible assets	8.293	3.299	8.162	3.278	8.502	3.323	-0.339***	-0.103
Log capital-labor ratio (machinery and equipment)	4.015	2.276	3.987	2.285	4.061	2.261	-0.074	-0.033
Log capital-labor ratio (tangible assets)	4.934	2.180	4.876	2.191	5.027	2.161	-0.151*	-0.069
Observations	3,278		2,016		1,262			

Notes: The table reports means and standard deviations of the log-transformed variables, separately by firm size band, for the full sample and for treated and untreated observations within each band. The Difference column reports the treated minus untreated gap in means; stars are based on a two-sample t-test allowing for unequal variances. The normalized difference is the difference in means divided by the square root of the average of the two group variances, following Imbens and Rubin (2015); values below 0.25 in absolute terms are conventionally regarded as small enough for linear regression adjustment to be reliable. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

However, in August 2020, a subsequent reform redefined regional subsidy eligibility at the county level, recognizing that counties within the same province may differ significantly in their economic development. As a result of this reclassification, 98 counties, previously included in Regions 1 to 3 due to their provincial affiliation, were reclassified as Region 4 based on local economic indicators (Table A.1). While these counties became eligible for the 6-point subsidy after 2020, they were ineligible during our sample period of 2006–2019. These counties are listed in in Appendix Table A.1.

The districts are also classified in six development levels, taking into account rankings and the natural breaks of the index scores of the districts. According to the results of 2022, there are 67 districts in Region 1, 173 in Region 2, 175 in Region 3, 215 in Region 4, 222 in Region 5 and 121 in Region 6 (Figure 2).

Figure 2. Socio-economic development levels by counties in Turkey



Source: General Directorate of Development Agencies (GDDA) (2023)

This temporal and spatial variation in policy rollout provides a credible source of exogenous variation in subsidy exposure. Specifically, the newly eligible counties, though similar in socioeconomic development to the earlier treated counties, were not eligible for the 6-point subsidy during our study window (2006–2019), making them well-suited as a control group. In contrast, the treatment group consists of firms located in provinces classified as Region 4, which began receiving the subsidy following the 2016 reform.⁸

⁸ We exclude Regions 5 and 6, as the parallel trends assumption before the treatment does not hold for these regions

To improve the comparability of the treatment and control groups, we implement additional sample refinements. First, we exclude seven counties⁹ from the control group that were reclassified from Region 4 to Region 3 based on the most recent Socio-Economic Development Index (SEGE, 2022). These counties no longer meet the underdevelopment criteria applied during the study period. Second, we exclude from the treatment group 136 counties classified as Regions 1–3 in terms of development score, but which were included in the subsidy program solely because they belonged to an underdeveloped province. This adjustment mitigates the risk of misclassifying economically stronger counties as treated and enhances the internal validity of our comparison. By refining both the treatment and control samples in this way, we reduce heterogeneity in baseline development levels, ensuring that our estimates capture the effect of subsidy eligibility rather than underlying differences in regional economic conditions.

In order to show that control group is similar to the treated regions in terms of socio-economic development levels, we also run a simple means in Table 2. We find the difference between the mean of SEGE of control and treatment groups are statistically insignificant, while that of between the control and non-treatment group is statistically significant.

Table 2. Statistical significance of mean differences between groups

	Treated vs. Control				Non-treated vs. Control			
	Mean (Treated)	Mean (Control)	Mean difference	p-value	Mean (Not-treated)	Mean (Control)	Mean difference	p-value
2004	-0.26	-0.24	-0.02	0.74	1.37	-0.24	1.61***	0.000
2017	-0.34	-0.31	-0.03	0.50	0.76	-0.31	1.07***	0.000
2022	-0.40	-0.45	0.05	0.31	0.71	-0.45	1.16***	0.000

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

It is important to note that our DiD estimates should be interpreted as Intention-To-Treat (ITT) effects. The EIS dataset does not contain information on whether individual firms actually received the subsidy, but only indicates their location, which determines eligibility for the program. This is also where our study departs from Aşık et al. (2022), as their data source includes information on whether a given firm has actually received the 6-points subsidy. Therefore, they present ATT and LATE effects on employment whereas our estimates capture the average effect of being exposed to treatment, rather than the effect of actual take-up.

This distinction is common in program evaluation settings where compliance is unobservable or imperfect, but treatment eligibility is exogenous and well-defined. As Bloom (2008) notes, ITT analysis is not only statistically valid under such conditions, but also policy-relevant, since policymakers often control eligibility and access, but not firm-level participation decisions. In this sense, our estimates reflect the real-world effect of the policy's expansion on eligible firms, regardless of take-up, and are therefore more generalizable for evaluating subsidy design and coverage.

⁹ These counties (affiliated province) are Kulu (Konya), Havza (Samsun), Yeniçağa (Bolu), Ferizli (Sakarya), İncirliova (Aydın), Nizip (Gaziantep), Gökçeşey (Zonguldak).

We acknowledge potential challenges to the validity of our identification strategy. The core assumption underlying the DiD framework is that, in the absence of the policy reform, the treatment and control groups would have followed parallel trends in the outcomes of interest. If this assumption does not hold, estimated treatment effects may be biased due to differential pre-trends or unobserved shocks.

To assess the plausibility of the parallel trends assumption, we implement event study specifications for firm employment (L), capital stock (K), and capital–labor ratio (K/L), which allows us to visually and statistically examine pre-treatment dynamics. These results, presented in the next section, provide supporting evidence that treatment and control groups were evolving similarly prior to the 2016 subsidy expansion, thereby reinforcing the credibility of our DiD estimates.

To estimate the causal effect of the 2016 subsidy reform, we employ a DiD regression model of the following form:

$$Y_{i,t} = \beta_0 + \beta_1(\text{Treated}_{i,t} \times D2016_t) + \beta_2(\text{Treated}_{i,t} \times D2013_t) + \phi_i + \phi_s + \tau_t + \epsilon_{i,t} \quad (1)$$

where i , t , r stand for firm, year, region, respectively. $Y_{i,t}$ denotes the outcome variable for firm i in year t , $(\text{Treated}_{i,t} \times D2016_t)$ is the treatment interaction term, equal to 1 for firms in treated counties and years after 2016. We also include an interaction between treatment status and an indicator for the post-2013 period, which absorbs the effect of the original six-point subsidy introduced under Law No. 6486 in 2013 and ensures that β_1 isolates the incremental effect of the March 2016 extension rather than conflating it with the earlier scheme. ϕ_i captures firm fixed effects, ϕ_s denotes technology-level fixed effects, τ_t represents year fixed effects, and $\epsilon_{i,t}$ is the idiosyncratic error term. We cluster error terms at the firm-level.

The coefficient of interest is β_1 , which captures the average treatment effect of the 2016 policy change, i.e., the introduction of eligibility for the additional 6-point subsidy on the outcome variables. The dependent variables include: the logarithm of firm-level employment (L), the logarithm of capital stock (K), the capital–labor ratio (K/L), and the logarithm of employment and employment share disaggregated by skill levels (proxied using ISCO-based occupation categories).

To account for potential confounding variation, we further control for province-specific linear time trends and 4-digit industry-by-year fixed effects, which help relax the strict parallel trends assumption by allowing for differential temporal patterns across regions and sectors. Industry-by-year fixed effects absorb any shock common to all firms in the same four-digit industry in a given year. This matters because treated and control counties might differ in industrial composition, and follow different trajectories over 2006 to 2019. With these terms included, the comparison is between treated and control firms operating in the same four-digit industry in the same year, so common industry shocks cannot generate spurious effects. Region-specific linear trends allow each region its own linear trajectory in the outcome and require only that treated and control firms follow common trends after that regional divergence is netted out.

This accommodates the possibility that the West and the East of the country were on structurally different growth paths throughout the period, which is likely given the development levels Figures 1 and 2 display.

4. Empirical Results

4.1. *Employment (L)*

We begin the empirical analysis by examining the impact of the 2016 subsidy expansion on firm-level employment in the manufacturing sector. Given that the primary policy goal of the additional 6-point employer-side subsidy was to stimulate job creation in underdeveloped regions, we focus first on how the policy affected the employment trajectories of firms, disaggregated by size. To isolate the treatment effect on existing businesses, we restrict the sample to firms that were already established before 2016, thereby shutting down potential firm entry effects. The analysis covers micro, small, and medium-to-large firms separately.

Figure 3 provides an event study specification that plots the dynamic treatment effects over time. In Panel A, micro firms show a clear upward shift in employment after 2016, with statistically significant and sustained gains relative to the control group. The confidence intervals tighten post-reform, further reinforcing the stability of the estimates. In contrast, Panel B (small firms) and Panel C (medium-large firms) show more muted responses, with coefficient estimates fluctuating around zero and confidence intervals that cross the null line. Furthermore, parallel trends assumption for employment outcomes does not seem to hold for small firms. These patterns imply that the employment response to the policy was largely and credibly confined to micro firms, which were newly exposed to the 6-point subsidy following the 2016 expansion.

Figure 3. Event study estimates of firm employment growth in treated region 4 counties

Panel A. Employment growth in micro firms

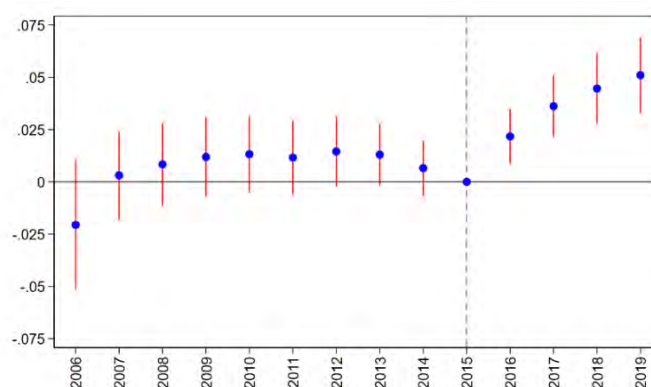
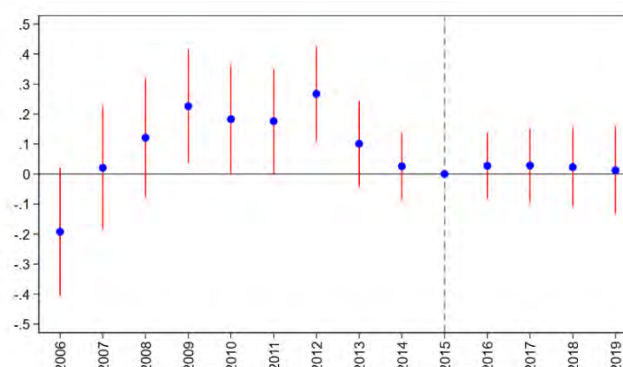
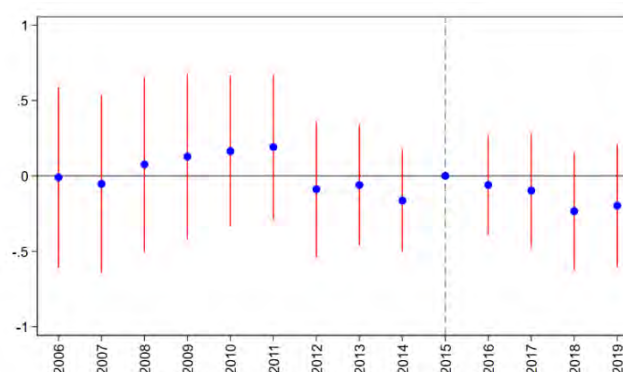


Figure 3. Event study estimates of firm employment growth in treated region 4 counties (continued)

Panel B. Employment growth in small firms



Panel C. Employment growth in medium and large firms



Source: Own estimations using EIS Database. The dependent variable is firm level employment (in natural logs). The figures represent the regression coefficient plots of the interaction between firms in treated counties and year dummies. All estimations include firm fixed effects and year fixed effects. The vertical lines for each coefficient show 95 percent confidence intervals, clustered at the firm level.

Table 3 presents the main DiD estimates of the policy's impact on log employment. The findings reveal a statistically significant and positive effect for micro firms, with a DiD coefficient of 0.025. This implies that micro firms in treated regions experienced, on average, a 2.5% increase in employment relative to comparable firms in untreated regions following the policy reform.¹⁰ For small firms, the coefficient is negative but statistically insignificant, suggesting no robust effect of the subsidy on this group. Similarly, for medium and large firms, the estimated effect is positive but also statistically insignificant. These results confirm that the employment effect of the policy was concentrated among the smallest firms, consistent with the design of the reform, which newly extended eligibility to firms with fewer than 10 employees.

¹⁰ This finding is consistent with the findings of Aşık et al. (2022).

Table 3. Firm employment in treated region 4 counties

	Micro	Small	Medium-Large
DID Coefficient	0.0250*** (0.00633)	-0.0621 (0.0668)	0.0828 (0.230)
Observations	182,797	16,366	3,278
R-squared	0.737	0.590	0.652
Year FE	Yes	Yes	Yes
4 Digit Industry Year FE	Yes	Yes	Yes
Nuts2 Linear Trends	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: Robust standard errors are in parenthesis. Clustering is at the firm level. Outcome variable is natural log of firm employment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

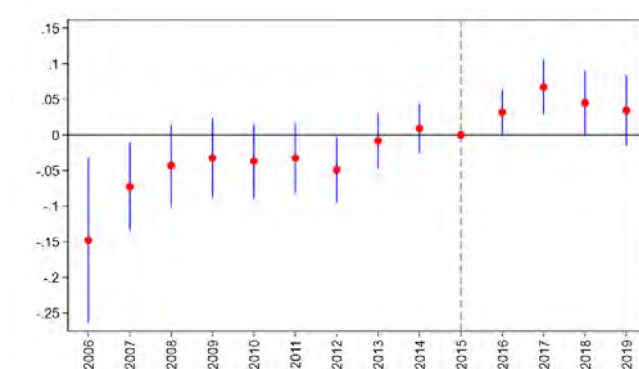
4.2. Capital accumulation (K)

We next investigate whether the 2016 subsidy expansion affected firms' capital accumulation, which serves as a key mechanism through which employment subsidies might generate longer-term productivity gains. In line with the previous theoretical discussions, employment subsidies can alter the marginal rate of substitution between labor and capital, potentially encouraging firms to either substitute labor for capital or invest more to complement expanded labor capacity. In this analysis, we use two distinct measures of capital; i) *Tangible Assets*, which captures the firm's entire stock of physical capital (land, buildings, machinery, vehicles, fixture), and, ii) *Machinery and Equipment*, a narrower but more directly productivity-linked capital input.

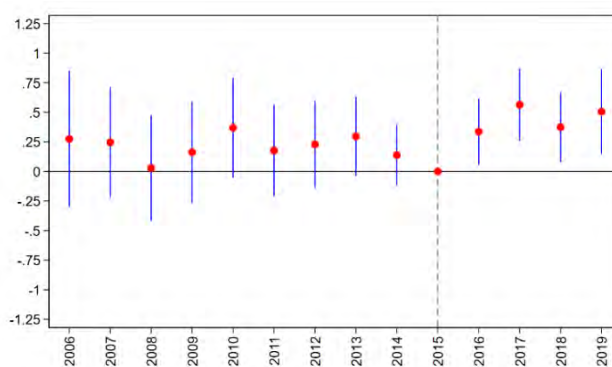
Figure 4 (Panels A-C) displays the dynamic treatment effects on total tangible assets by firm size. This time, the pre-treatment coefficients for small and medium-large firms are individually indistinguishable from zero and show no discernible trend towards the reform, which is what the parallel trend assumption requires (Panels B-C). However, in Panel A, this is not true for micro firms although they exhibit a visible change in the trend of capital accumulation following the policy. Nevertheless, the wide confidence intervals surrounding the estimates make it difficult to interpret this as a statistically significant effect. In Panel B, small firms show a gradual and statistically significant increase in capital stock after 2016, suggesting a positive response to the policy reform. In contrast, Panel C indicates that medium and large firms followed a relatively flat trajectory with broad confidence bands, signaling no robust treatment effect. Figure 5 presents the corresponding event study analysis for machinery and equipment investments. None of the firm types exhibit any systematic post-treatment deviation from baseline trends for the machinery and equipment investment.

Figure 4. Event study estimates of firm capital stock growth in treated region 4 counties, (K: total tangible assets, in logs)

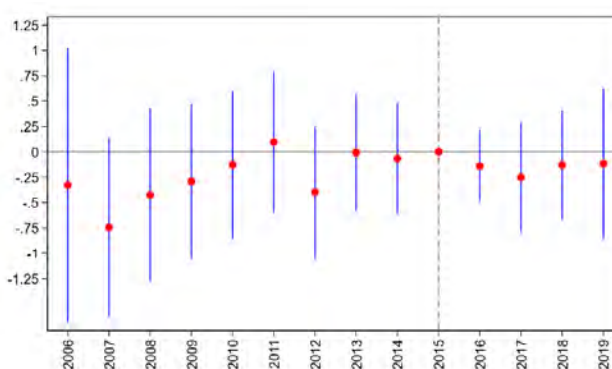
Panel A. Capital stock growth in micro firms



Panel B. Capital stock growth in small firms



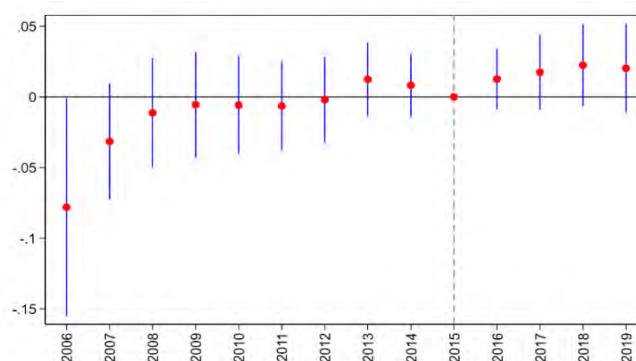
Panel C. Capital stock growth in medium and large firms



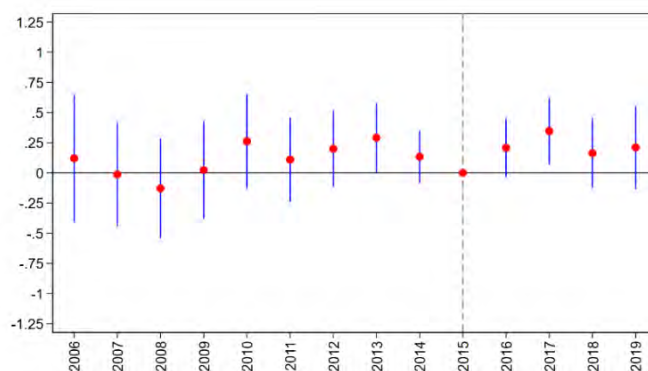
Source: Own estimations using EIS Database. The dependent variable is firm level tangible assets (in natural logs). The figures represent the regression coefficient plots of the interaction between firms in treated counties and year dummies. All estimations include firm fixed effects and year fixed effects. The vertical lines for each coefficient show 95 percent confidence intervals, clustered at the firm level.

Figure 5. Event study estimates of firm machinery growth in treated region 4 counties, (K: machinery and equipment, in logs)

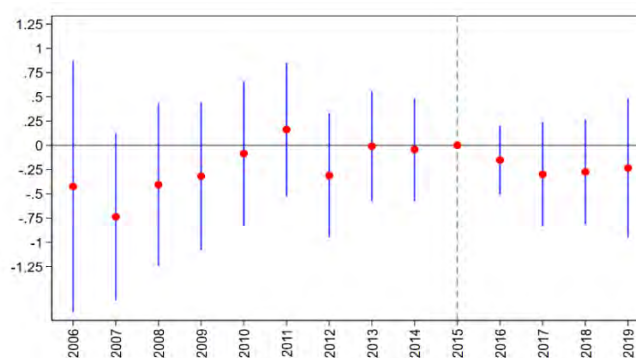
Panel A. Machinery growth in micro firms



Panel B. Machinery growth in small firms



Panel C. Machinery growth in medium and large firms



Source: Own estimations using EIS Database. The dependent variable is firm level machinery and equipment (in natural logs). The figures represent the regression coefficient plots of the interaction between firms in treated counties and year dummies. All estimations include firm fixed effects and year fixed effects. The vertical lines for each coefficient show 95 percent confidence intervals, clustered at the firm level.

Table 4 reports the DiD estimates of the policy's impact on the logarithm of capital stock, broken down by capital type and firm size. Panel A shows that the policy led to statistically significant increases in tangible assets for both micro and small firms, with the effect being more pronounced among small firms, consistent with the event study estimates. In contrast, no significant impact is observed for medium and large firms. Panel B, which focuses only on machinery and equipment as the outcome variable, reveals no statistically significant effects across any firm size category.

Table 4. Firm capital stock in treated region 4 counties

	Micro	Small	Medium-Large
Panel A: $K=Tangible\ Assets\ (in\ logs)$			
DID Coefficient	0.0310* (0.0179)	0.317** (0.145)	-0.0966 (0.262)
Observations	182,797	16,366	3,278
R-squared	0.805	0.752	0.728
Panel B: $K=Machinery\ and\ Equipment\ (in\ logs)$			
DID Coefficient	0.00787 (0.0115)	0.0908 (0.138)	-0.363 (0.356)
Observations	182,797	16,366	3,278
R-squared	0.806	0.808	0.770
Year FE	Yes	Yes	Yes
4 Digit Industry Year FE	Yes	Yes	Yes
Nuts2 Linear Trends	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: Robust standard errors are in parenthesis. Clustering is at the firm level. Outcome variable is natural log of firm tangible assets (Panel A) or firm machinery and equipment (Panel B). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

These results suggest that small firms, though not directly targeted by the 2016 expansion, on average redirected resources toward non-productive capital accumulation whereas there is no evidence of behavior change for medium and large firms.

4.3. Capital intensity (K/L Ratio)

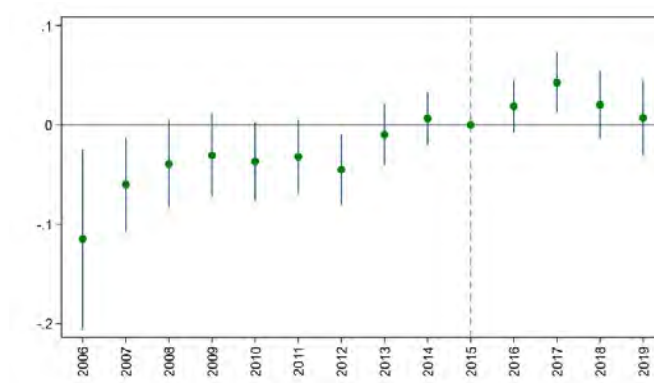
We next examine the impact of the 2016 subsidy expansion on capital intensity, measured as the ratio of capital stock to labor input (K/L). Capital intensity provides insight into how firms adjust their input mix in response to changes in labor costs. A decline in labor cost, induced by the subsidy may reduce capital intensity if firms substitute labor for capital or increase it if firms reinvest cost savings in productivity-enhancing assets alongside labor expansion. As in the previous section, we use the two definitions of capital, i.e., tangible assets and machinery-equipment.

Figure 6 shows the dynamic treatment effects for K/L based on total tangible assets. In Panel B, there is a clear post-2016 upward shift in capital intensity for small firms. In Panels A and C (micro and medium-large firms), however, the estimates remain close to zero with wide confidence intervals. In Figure 7, we plot K/L based on machinery and equipment only. This time, we find no statistically significant evidence of increase in capital intensity for small firms either as most post-treatment coefficients include zero.

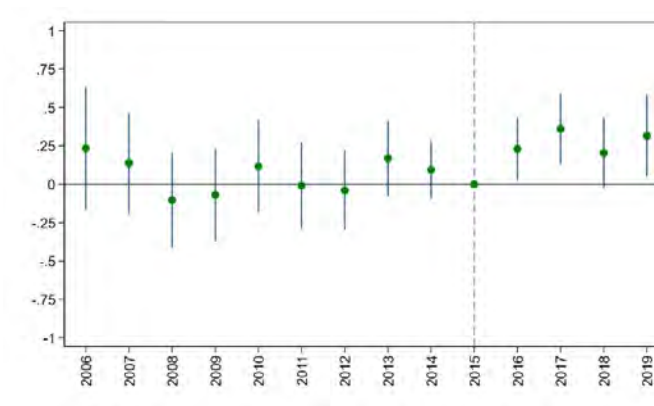
Figure 6. Event study estimates of firm capital intensity (K/L) in treated region 4 counties,

(K: Total Tangible Assets)

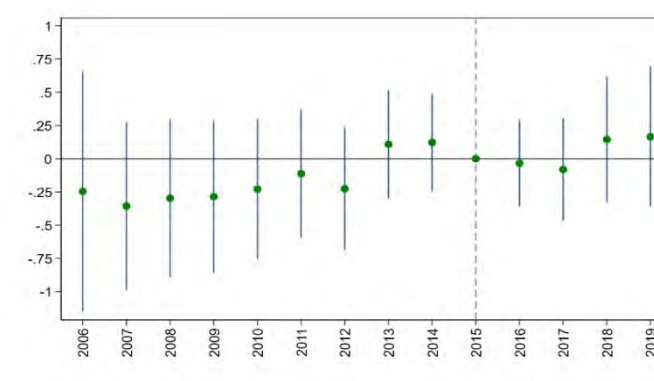
Panel A. Capital Intensity in Micro Firms



Panel B. Capital Intensity in Small Firms



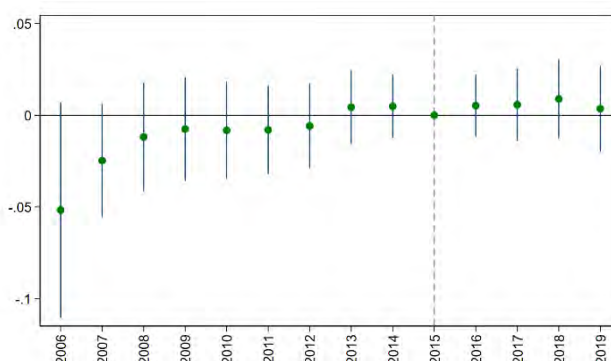
Panel C. Capital Intensity in Medium and Large Firms



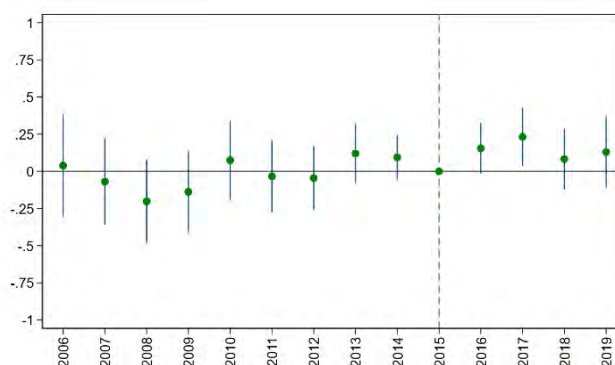
Source: Own estimations using EIS Database. The dependent variable is firm level tangible assets divided by number of workers (in natural logs). The figures represent the regression coefficient plots of the interaction between firms in treated counties and year dummies. All estimations include firm fixed effects and year fixed effects. The vertical lines for each coefficient show 95 percent confidence intervals, clustered at the firm level.

Figure 7. Event study estimates of firm capital intensity (K/L) in treated region 4 counties, (K: machinery and equipment)

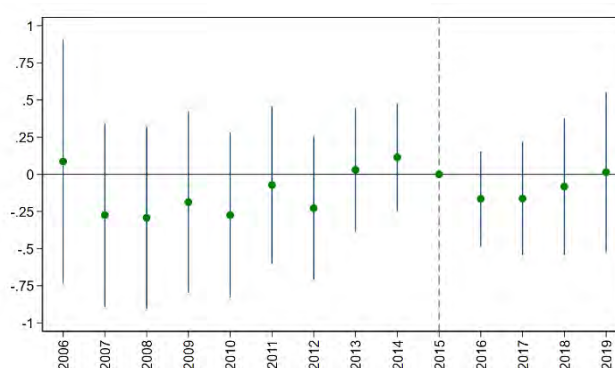
Panel A. Capital intensity in micro firms



Panel B. Capital intensity in small firms



Panel C. Capital intensity in medium and large firms



Source: Own estimations using EIS Database. The dependent variable is firm level machinery and equipment divided by number of workers (in natural logs). The figures represent the regression coefficient plots of the interaction between firms in treated counties and year dummies. All estimations include firm fixed effects and year fixed effects. The vertical lines for each coefficient show 95 percent confidence intervals, clustered at the firm level.

Table 5 reports the effect of the subsidy on capital intensity, measured as the log capital-labor ratio, separately by firm size and for two definitions of capital. The response is confined to small firms and to the broader measure of capital. In Panel A, treated small firms increased tangible assets per worker relative to control firms, and the effect is significant at the five percent level. The corresponding estimate for machinery and equipment in Panel B is positive but not statistically distinguishable from zero, so the capital deepening we observe operates through tangible assets broadly rather than through equipment specifically. For micro firms the estimates are close to zero on both measures and precisely estimated, allowing us to rule out capital intensity responses of any economically meaningful size in this group. For medium and large firms both coefficients are negative, but the standard errors are large and neither approaches conventional significance, which is unsurprising given the small number of observations in this panel. Taken together, the evidence suggests that capital deepening occurred selectively, only at small firms, but for tangible assets not the machinery and equipment. For micro firms, the absence of a capital response alongside employment growth likely reflects a labor-intensive production model or limited access to investment finance.

Table 5. Firm capital intensity (K/L) in treated region 4 counties

	Micro	Small	Medium-Large
Panel A: $K=\text{Tangible Assets (in logs)}$			
DID Coefficient	0.0179 (0.0132)	0.240** (0.106)	-0.0770 (0.189)
Observations	182,797	16,366	3,278
R-squared	0.793	0.742	0.711
Panel B: $K=\text{Machinery and Equipment (in logs)}$			
DID Coefficient	0.00253 (0.00828)	0.102 (0.0943)	-0.176 (0.208)
Observations	182,797	16,366	3,278
R-squared	0.792	0.794	0.752
Year FE	Yes	Yes	Yes
4 Digit Industry Year FE	Yes	Yes	Yes
Nuts2 Linear Trends	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: Robust standard errors are in parenthesis. Clustering is at the firm level. Outcome variable is capital intensity (K/L) measured by firm tangible assets (Panel A) or firm machinery and equipment (Panel B). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

4.4. Heterogeneity check by pre-treatment average firm size

To further test the robustness of the findings, we reclassify firms by their average employment during the pre-treatment period (2006–2015). This alternative classification allows us to examine which firms relative to the original eligibility threshold of 10 employees were most affected by the 2016 reform. This design helps address potential misclassification caused by temporary employment fluctuations or firm growth and isolates groups more or less likely to be newly treated. Firms are grouped into four size bands based on their pre-reform average employment; i) 1–4 employees: clearly ineligible before 2016 (micro firms), ii) 5–7 employees: likely close to the eligibility threshold, mostly ineligible, iii) 8–12 employees: near or just above threshold; potentially marginally eligible, iv) 13+ employees: most likely to be eligible pre-reform.

The results are presented in Table 6, with each row corresponding to a size band and columns reporting DiD estimates for: Log employment (Column 1), Log capital as total tangible assets (Column 2), Log capital as machinery and equipment (Column 3), Capital-labor ratio (K/L)

with total tangible assets (Column 4) and Capital-labor ratio (K/L) with machinery and equipment (Column 5).

The first group consisting of firms with 1 to 4 employees shows the strongest and most consistent policy response across all outcomes. Employment rises by 4.93 percent, and capital stock increases significantly by 9.96 percent for tangible assets and 5.76 percent for machinery. Capital intensity also improves, with statistically significant gains in the K/L ratio using tangible assets. These findings challenge the assumption that the smallest micro firms are too resource-constrained to invest and instead suggest that subsidy access can meaningfully enable both labor and capital expansion. The second group with employment size 5 to 7 exhibits positive but statistically insignificant effects across all outcomes. These firms may have been near or partially eligible before the reform, leading to more muted responses. As for the third group (employment size 8-12) which operate close to the threshold employment eligibility shows statistically significant gains in capital stock and capital intensity, using tangible assets. The coefficient for tangible assets is positive and significant (at the 5 percent level), and the K/L ratio (based on the same definition) also increases significantly. This indicates that marginally eligible firms responded on average by investing in broad physical capital, even if employment effects were not statistically significant. The response suggests capital deepening in firms that were already relatively close to full eligibility. And finally for the last group, there is no statistically significant effect on any outcomes. These firms were most likely to be eligible before the reform, and their stable post-2016 outcomes serve as a useful placebo check, supporting the validity of the identification strategy.

Table 6. Employment, capital and capital intensity (K/L) in treated region 4 counties

	Employment	Tangible Assets	Machinery & Equipment	K/L (Tang. Assets)	K/L (Mach.&Eqp.)	Obs.
Firm size: 1-4 employees						
DID Coefficient	0.0493*** (0.0167)	0.0996** (0.0478)	0.0576* (0.0312)	0.0577* (0.0350)	0.0348 (0.0231)	63,630
Firm size: 5-7 employees						
DID Coefficient	0.00167 (0.0629)	-0.0103 (0.176)	-0.121 (0.128)	-0.0112 (0.120)	-0.0461 (0.0868)	9,254
Firm size: 8-12 employees						
DID Coefficient	0.0409 (0.105)	0.567** (0.266)	0.247 (0.244)	0.389** (0.190)	0.195 (0.162)	5,869
Firm size: 13+ employees						
DID Coefficient	0.0285 (0.0921)	0.139 (0.147)	-0.0458 (0.166)	0.0852 (0.0996)	-0.0245 (0.103)	23,116
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
4 Digit Industry Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Nuts2 Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Industry-year fixed effects are at 4-digit industry level. Standard errors are clustered at the firm level. Robust standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

This heterogeneity analysis confirms that the employment subsidy reform had differentiated impacts depending on firms' pre-policy proximity to eligibility thresholds. While the strongest effects are observed in the smallest micro firms (1-4 employees), firms with 8-12 employees also showed significant capital responses, particularly in tangible asset accumulation and

capital deepening. These patterns reinforce the finding that relaxing eligibility constraints for small firms can generate multi-dimensional firm growth, both along labor and capital margins.

4.5. Robustness check

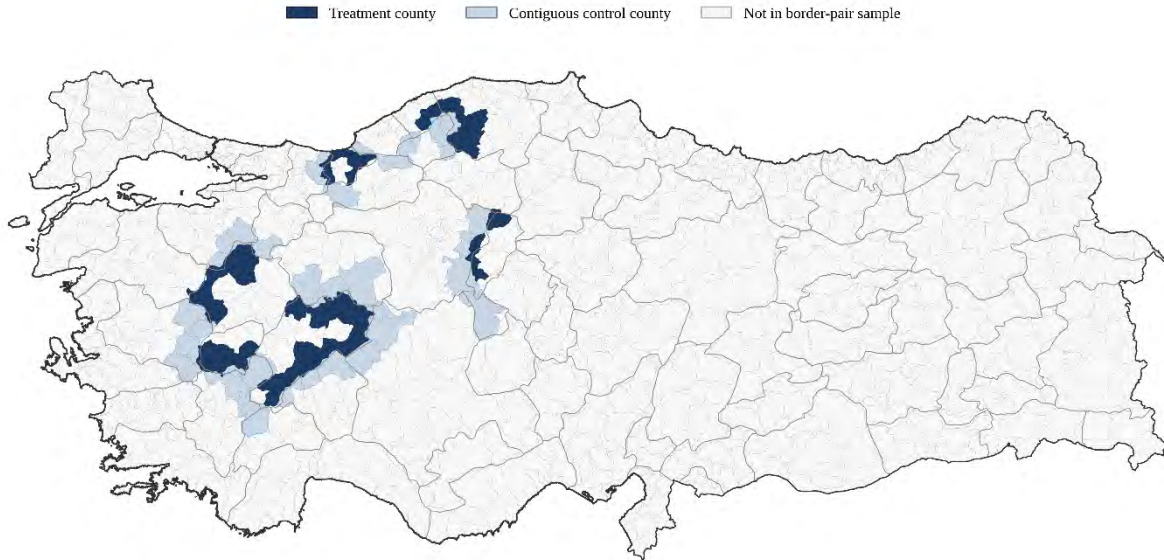
The identifying assumption behind our baseline estimates is that treated and control firms would have followed parallel trajectories in the absence of the 2016 reform. The principal threat to this assumption in our setting is geographic. Although the control counties were reclassified into Region 4 in 2020 and are comparable to the treated counties in measured development, as Table 2 confirms, they remained within provinces classified in Regions 1 to 3 during our sample period. They may therefore be a part of provincial economies with different sectoral compositions, different exposure to national and international shocks, and different underlying growth paths. While our NUTS-2 linear trends and 4-digit industry-by-year fixed effects relax the strict parallel trends requirement, they do so parametrically and cannot absorb local shocks that evolve non-linearly.

We therefore implement a border-pair design that addresses this concern non-parametrically, by narrowing the comparison to treated and control firms in directly adjacent counties. The approach follows Dube, Lester and Reich (2010), who compare all contiguous county pairs straddling a state border in the United States and show that specifications ignoring local economic conditions generate spurious estimates through spatial heterogeneity in employment trends, and it applies the cross-border logic first used by Card and Krueger (1994). The premise is that firms on either side of a subsidy boundary face common local conditions, so that the boundary generates variation in eligibility that is plausibly exogenous while local shocks are shared across the two sides. The design is feasible only where a treated county physically adjoins one of the counties in our control group, namely those reclassified into Region 4 in 2020. Region 6 is bounded domestically only by Region 4 and Region 5 provinces and so contributes no such adjacencies. We accordingly restrict the analysis to Region 4 treatment counties adjoining a control county, and exclude provincial capitals on both sides of the boundary. The resulting sample is displayed in Figure 8 and listed in full in Appendix Table A.2.

We pool all contiguous treatment-control county pairs into a single regression that augments our baseline specification with pair-by-year fixed effects. These absorb any shock common to a given pair in a given year, including weather, movements in local input costs and fluctuations in regional demand, without imposing any functional form on how such shocks evolve. Identification therefore derives entirely from the contrast between treated and control firms observed within the same pair and the same year. Because a county bordering several counties on the opposite side enters more than one pair, observations for such counties are replicated across pairs. A natural concern in any border design is that proximity makes control units susceptible to contamination, since a subsidy tied to firm location creates an incentive to establish activity on the eligible side of a nearby boundary. This concern is muted in our setting because the sample is restricted throughout to firms already operating before March 2016,

which shuts down entry, and because treatment status is assigned according to each firm's county of location prior to the reform as recorded in year 2015, so that subsequent changes in location do not reclassify a firm. The residual channel is the redistribution of activity by incumbent firms across existing establishments. Since the control and treatment firms are now different than our preferred identification strategy, the estimating sample size is different than in our baseline. Results are reported in Table 7.

Figure 8. Border-county pairs



As Table 7 shows, the capital deepening response of small firms documented in Section 4.2 survives this more demanding comparison, while the employment response of micro firms reported in Section 4.1 does not. For micro firms, the estimated ITT coefficient in column (1) of Panel A is now insignificant and the employment response documented in Table 3 is no longer identified once treated firms are compared exclusively with control firms in adjacent counties. Micro firms display no response on any other margin either: the coefficients on tangible assets, machinery and equipment, and both capital-labor ratios are uniformly small, three of the four are negative, and none approaches conventional significance. Taken together, the border-pair evidence indicates that the estimated micro firm employment gain is sensitive to the composition of the control group and, more specifically, to whether local economic conditions are held fixed at the treatment boundary.

Table 7. Robustness check: control groups restricted to firms in border counties

	Micro	Small	Medium-Large
	(1)	(2)	(3)
EMPLOYMENT (in logs)			
<i>Panel A: Firm Employment</i>			
DID Coefficient	0.001 (0.0119)	0.142 (0.103)	-0.282 (0.266)
CAPITAL STOCK (in logs)			
<i>Panel B: K=Tangible Assets</i>			
DID Coefficient	-0.0268 (0.0311)	0.454** (0.208)	0.0239 (0.328)
<i>Panel C: K=Machinery and Equipment</i>			
DID Coefficient	-0.00347 (0.0165)	0.135 (0.206)	-0.203 (0.416)
CAPITAL LABOR RATIO (in logs)			
<i>Panel D: K=Tangible Assets</i>			
DID Coefficient	-0.0208 (0.0233)	0.274* (0.163)	0.410* (0.238)
<i>Panel E: K=Machinery and Equipment</i>			
DID Coefficient	-0.00390 (0.0117)	0.0664 (0.134)	0.234 (0.250)
Observations	158,229	17,209	5,471
Firm Fixed Effects	Yes	Yes	Yes
4 Digit Industry Fixed Effects	Yes	Yes	Yes
Border Province-Year Fixed Effects	Yes	Yes	Yes
NUTS2 Fixed Effects	Yes	Yes	Yes
Pair County-Year Fixed Effects	Yes	Yes	Yes

Notes: Standard errors are clustered at the firm level. Robust standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The list of contiguous treatment and control counties is provided in Table A.2.

For small firms the baseline finding survives this more demanding comparison. The coefficient on tangible assets in column (2) of Panel B is 0.454 and significant at the five percent level, exceeding the corresponding baseline estimate of 0.317. The capital-labor ratio in tangible assets, reported in Panel D, is 0.274 and significant at ten percent, again slightly larger than its baseline counterpart. Machinery and equipment remains statistically indistinguishable from zero in both the capital stock and capital intensity specifications. The asymmetry between broad tangible assets and machinery therefore persists, and indeed becomes more pronounced, when identification is restricted to within-pair-year variation. Employment among small firms also remains insignificant, consistent with the baseline. Overall, the capital response strengthens under a specification designed to purge spatially correlated shocks provides reassurance that the statistically significant findings for small firms are not driven by differential regional trends. As for medium and large firms (Column 3), the estimated coefficients are once again largely insignificant.

4.6. Skill composition of labor

We next examine whether the 2016 subsidy expansion affected the skill composition of the workforce. Given the policy's design as a labor cost-reduction tool primarily targeted at formalization and job creation, its potential to induce shifts in the skill profile of employment is theoretically ambiguous. On one hand, lower labor costs could allow firms to hire more skilled workers at reduced effective wages. On the other hand, subsidies may encourage employment of lower-skilled or informal workers, particularly in labor-intensive firms.

The International Standard Classification of Occupations, maintained by the International Labor Organization and most recently revised as ISCO-08, organizes occupations into ten major groups identified by the first digit of the occupational code. Major group 1 comprises managers, group 2 professionals, group 3 technicians and associate professionals, group 4 clerical support workers, group 5 service and sales workers, group 6 skilled agricultural, forestry and fishery workers, group 7 craft and related trades workers, group 8 plant and machine operators and assemblers, and group 9 elementary occupations.¹¹ The groups are ordered by skill level, with groups 1 to 3 corresponding broadly to high-skill occupations, groups 4 to 8 to medium-skill occupations, and group 9 to low-skill occupations, which makes the first digit a convenient basis for constructing measures of workforce skill composition. Using these classifications, we define our outcome variable as the share of skilled labor in total employment.

Table 8 provides the results for total skilled labor, managers, professionals, technicians, and craftsmen. Across all firm size categories, micro, small, and medium-large, none of the estimated treatment effects are statistically significant at conventional levels. While some coefficients are positive (e.g., for technicians in micro firms, or total skilled labor in small firms), standard errors are large, and no consistent pattern emerges. These null findings suggest that the subsidy did not materially affect the skill composition of employment. This is consistent with our earlier conclusion that employment gains were concentrated in micro firms, which tend to operate with low-skilled labor and may have limited capacity or incentive to shift toward higher-skilled personnel. It also supports concerns in the literature (e.g., Oskamp and Snower, 2006) that employment subsidies can reduce incentives for skill upgrading, especially when tied to low-wage or informal-sector transitions. The absence of significant effects on skill upgrading implies that the 2016 subsidy expansion might have functioned primarily as a job quantity intervention, rather than one that improved job quality or workforce structure.¹²

Table 8. The share of skilled labor employment in treated region 4 counties

	Total Skilled	Managers	Professionals	Technicians	Craftsmen
Micro firms					
DID Coefficient	0.0011 (0.0061)	-0.0003 (0.0022)	0.0031 (0.0036)	0.0014 (0.0058)	-0.0124 (0.0132)
Small firms					
DID Coefficient	-0.0026 (0.0180)	0.0053 (0.0062)	-0.0068 (0.0124)	-0.0079 (0.0171)	-0.0215 (0.0279)
Medium-large firms					
DID Coefficient	-0.0074 (0.0422)	0.006 (0.009)	-0.0402 (0.0234)	-0.0134 (0.0422)	-0.0105 (0.0689)
Year FE	Yes	Yes	Yes	Yes	Yes
4 Digit Industry Year FE	Yes	Yes	Yes	Yes	Yes
Nuts2 Linear Trends	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes

Notes: Industry-year fixed effects are at 4-digit industry level. Standard errors are clustered at the firm level. Outcome variable is the share of employment in total employment. Robust standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

¹¹ A tenth group, coded 0, covers armed forces occupations.

¹² The null effects persist when we include levels rather than shares in total employment.

In sum, the empirical analysis reveals a nuanced but consistent pattern in the impact of the 2016 employment subsidy reform on Turkish manufacturing firms. While micro firms experienced significant increases in employment, small firms responded primarily through increased capital investment, particularly in tangible assets rather than machinery and equipment and enhanced capital intensity given this definition of the capital. These effects were absent for medium and large firms, suggesting the policy's impact was concentrated among previously ineligible or marginally exposed firms. The policy appears to have had no discernible effect on skill composition, as estimates for the skilled labor share are statistically insignificant. Overall, the results indicate that the 2016 subsidy expansion functioned effectively as a quantity-focused intervention, stimulating employment and capital accumulation among small firms, without triggering structural upgrading in the workforce.

4.7. Profitability and other firm outcomes

In the final component of our analysis, we use firms' income statements to explore whether the reduction in labor costs was reflected as higher earnings or passed through into other margins of adjustment (Table 9). We are considering four measures. Gross profits, defined as revenue less the cost of goods sold, capture the effect closest to the cost shock itself, since the employer's social security contribution enters directly into production costs. Operating profits deduct administrative and marketing expenses and therefore reflect the firm's performance in its core activity. Earnings before interest, taxes, depreciation and amortization (EBITDA) further excludes depreciation and amortization, which is useful in our setting because firms undertaking capital investment mechanically report higher depreciation charges in subsequent years. And finally, net profits, are the residual accruing to the firm after interest and taxation. We complement these levels with two ratios, the gross profit margin and the ratio of net profits to sales, which express profitability relative to the scale of the firm's activity and are therefore comparable across firms of different sizes. It is important to note that the number of observations radically change due to missing data, especially for micro firms.

Table 9. Firm profitability in treated region 4 counties

	Micro	Small	Medium-Large
Panel A: Gross real profits			
DID Coefficient	-46.81* (26.26)	-53.04 (179.3)	5,835 (4,491)
Observations	24,738	13,415	2,973
R-squared	0.645	0.709	0.886
Panel B: Operating real profits			
DID Coefficient	-13.52 (15.96)	-37.81 (91.09)	3,045 (3,370)
Observations	24,744	13,416	2,973
R-squared	0.522	0.627	0.820
Panel C: Real EBITDA			
DID Coefficient	-71.77 (47.38)	-114.6 (143.6)	21,185** (9,981)
Observations	23,312	13,174	2,964
R-squared	0.975	0.761	0.945
Panel D: Net real profits			
DID Coefficient	-0.952 (13.33)	-108.0 (97.61)	2,599 (2,702)
Observations	24,744	13,416	2,973
R-squared	0.481	0.544	0.759
Panel E: Gross profit margin			
DID Coefficient	0.105 (0.0742)	0.0539 (0.0468)	-0.00468 (0.0314)
Observations	20,029	12,239	2,808
R-squared	0.476	0.436	0.786
Panel F: Net profits to sales			
DID Coefficient	-216.8 (232.8)	43.41 (32.41)	1.692 (1.840)
Observations	20,042	12,242	2,808
R-squared	0.306	0.200	0.479
Year FE	Yes	Yes	Yes
4 Digit Industry Year FE	Yes	Yes	Yes
Nuts2 Linear Trends	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: Robust standard errors are in parentheses. Clustering is at the firm level. Profit measures are expressed in real terms, deflated by NACE-2 sectoral deflators and are not log-transformed because profits take negative values for a substantial share of firms. Gross profit margin and net profits to sales are ratios. All specifications include firm, year and four-digit industry-by-year fixed effects together with NUTS2 linear trends. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The aim of examining these outcomes is to shed light on firm behavior and the margins of adjustment. A subsidy that lowers the marginal cost of labor can be absorbed in several ways: firms may retain it as higher margin, or reinvest it in capital and labor inputs. As Table 9 displays, the subsidy produced no detectable effect on firm profitability. Across the four measures reported in Panels A to D, none of the coefficients are significant, except for gross real profits for micro firms and real EBITDA for medium and large firms. Two features of the estimation sample require comment. First, profit and sales variables are drawn from firms' balance sheet filings, which are available only for firms keeping accounts on the balance sheet basis. Coverage is therefore substantially lower than in our employment and capital specifications, and the shortfall is concentrated among the smallest firms: the micro sample falls from 182,797 firm-year observations to 24,744, whereas small firms retain 13,416 of 16,366 and medium and large firms 2,973 of 3,278. These figures imply that the coverage rates are 14 percent for micro firms, 82 percent for small firms and 91 percent for medium and large firms. Therefore, micro estimates should accordingly be read as applying to the subset of micro firms

that file full financial statements, which are systematically the larger and more formalised firms within that band. Secondly, the EBITDA result for medium and large firms is contradicted by the corresponding estimates for operating and net profits in the same size band, both of which are statistically indistinguishable from zero. The negative gross profit estimate for micro firms is likewise isolated, since operating and net profits for the same firms are close to zero and precisely estimated. Therefore overall, the evidence indicates that treated firms did not retain the reduction in labor costs as higher margin.

The null results on profitability along with the capital formation we show for small firms, might be suggestive that the cost saving were absorbed into input use rather than accumulated as margin. If treated firms had simply banked the cost saving, we would expect profitability to rise across the size distribution, and the capital results would be harder to attribute to the subsidy. Therefore, the evidence that profitability is unchanged narrows the set of channels through which the policy operated.

5. Conclusion

This study examines the firm-level effects of exposure to Turkey's 2016 employment subsidy reform, which significantly expanded the scope of existing labor cost support by granting a 6-percentage-point reduction in employer-side social security contributions to all employees of firms in underdeveloped regions. Using rich, longitudinal administrative data from the Entrepreneurship Information System (EIS) and a quasi-experimental difference-in-differences design, this study evaluates how firms adjusted not only their employment levels but also their capital investment, capital-labor ratios, and workforce composition in response to this policy change.

Our study aims to contribute to the literature by providing evidence on whether employment subsidies act merely as short-term job creation tools or whether they also lead to broader transformations in firm behavior. While prior studies have assessed the effects of such policies on formal employment, relatively little is known about their impact on capital formation, capital-labor substitution, or skill upgrading, particularly in developing and emerging economies. This study contributes to filling this gap by analyzing the heterogeneous effects of the reform across firms of different sizes and stages of formalization.

Our analysis reveals a clear pattern of size-based heterogeneity in firm responses to the policy. Micro firms, which had previously been excluded from the subsidy scheme, experienced statistically significant employment gains once exposure to eligibility was extended, responding on the extensive margin. However, this employment expansion was not accompanied by additional investments in productive capital, or capital-labor ratios, suggesting that micro firms used the subsidy primarily to scale labor rather than to transform production technology or input composition. In contrast, we show that small firms (10–49 employees) exhibited capital deepening in the form of tangible assets, with no significant effects on employment. These results suggest that small firms were more likely to reinvest cost savings into tangible asset

accumulation but not on machinery and equipment. In other words, firms may have invested in buildings, vehicles, or non-productive fixed assets rather than in productivity-enhancing capital. The impact was especially pronounced in firms near the eligibility threshold prior to the reform (e.g., firms with 8–12 pre-reform employees), pointing to the importance of marginal eligibility status in determining firm responsiveness.

Interestingly, medium and large firms, which benefited the most in absolute monetary terms after the policy shifted from new hires to all employees exhibited no statistically significant behavioral response in any of the outcomes analyzed. This suggests that for these firms, the policy functioned largely as a fiscal transfer or windfall, with no discernible effect on hiring, investment, or input mix.

Turning to the skill dimension, the subsidy appears to have had no significant impact on the skill composition of the workforce across all firm sizes as the share of skilled employment remained unchanged. This null result complements the non-responsiveness in machinery and equipment investment, reinforcing the interpretation that the policy did not induce structural transformation or workforce upgrading. The labor and capital adjustments that occurred were primarily extensive rather than qualitative.

Our study also shows that the subsidy left firm profitability unchanged across all size categories, on measures ranging from gross profits through operating profits and EBITDA to net profits. Taken together with the capital results for small firms, this suggests that the cost saving was absorbed into input use rather than retained as margin, although the profitability estimates rest on a substantially smaller sample owing to the limited availability of balance sheet filings among the smallest firms.

These findings carry important implications for both labor economics theory and public policy design. First, the asymmetric responses across firm sizes support theoretical models that highlight the role of credit constraints, production technologies, and marginal incentives in mediating subsidy effectiveness (Petrucci & Phelps, 2005; Crépon & Desplatz, 2003). For micro firms, labor subsidies act as a cost relief mechanism, enabling short-run hiring but without catalyzing more durable investment behavior. In contrast, small firms with more formalized structures may interpret the subsidy as a signal or opportunity for enhancing investment, particularly in tangible capital. Second, the lack of any observable skill upgrading or capital deepening in technology-intensive assets may suggest that subsidies alone are insufficient to push firms toward higher-value production. Without complementary policies, such as vocational training, digitalization incentives, or innovation support, wage subsidies may reinforce low-skill, low-productivity employment structures. This is especially critical in middle-income countries like Turkey, where wage compression, informality, and limited technological absorption remain key challenges.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this work, the author(s) used Claude AI for editing, English grammar check and coding for visualization. The author(s) reviewed and edited the output as needed and take full responsibility for the content of the published article.

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Appendix

Table A.1. Counties which were included under region 4 after 2020

Province	Region before 2020	County	Region after 2020
Adana	3	Yumurtalık, İmamoğlu, Karataş, Karaisalı	4
Ankara	1	Haymana, Bala, Çamlıdere	4
Aydın	2	Kuyucak, Germencik, Karacasu, Sultanhisar, Köşk, Buharkent, Yenipazar, İncirliova, Bozdoğan	4
Balıkesir	2	Savaştepe, Dursunbey, Sındırgı, Havran, Kepsut	4
Bilecik	3	Gölpazarı, İnhisar, Yenipazar	4
Bolu	2	Yeniçağa, Mudurnu, Göynük	4
Burdur	3	Kemer, Ağlasun, Çavdır, Çeltikçi, Yeşilova, Altınyayla	4
Bursa	1	Harmancık, Keles	4
Çanakkale	2	Bayramiç, Yenice	4
Denizli	2	Babadağ, Kale, Beyağaç, Baklan, Güney, Çameli	4
Edirne	2	Enez, İpsala, Meriç	4
Eskişehir	1	Alpu, Günyüzü	4
Gaziantep	3	Nizip, İslahiye, Oğuzeli, Nurdağı, Karkamış, Araban, Yavuzeli	4
Isparta	2	Şarkikaraağaç, Aksu	4
Karabük	2	Eskipazar, Yenice	4
Kayseri	2	İncesu, Felahiye, Yahyalı, Bünyan, Yeşilhisar, Pınarbaşı	4
Kırklareli	2	Demirköy, Pehlivan köy	4
Konya	3	Kulu, Sarayönü, Hadim, Taşkent, Güneysınır, Kadınhanı, Doğanhisar, Tuzlukçu, Yalıhöyük, Bozkır, Derebucak	4
Manisa	2	Saruhanlı, Köprübaşı, Ahmetli, Gölarmara, Selendi	4
Mersin	3	Aydıncık, Mut, Gülnar	4
Sakarya	2	Kaynarca, Ferizli	4
Samsun	3	Kavak, Havza, Alaçam, Yakakent, Salıpazarı, Vezirköprü, Asarcık, Ayvacık	4
Zonguldak	3	Kilimli, Gökçebeş	4

Notes: Adopted from Aşık et al. (2022). The table lists the 98 counties, located in 23 provinces classified in Regions 1 to 3 during the sample period, that were reclassified into Region 4 by the investment incentive scheme announced in 2020. These counties constitute the control group. Source: Official Gazette No. 31220, 21 August 2020.

Table A.2. List of control counties contiguous to counties eligible for the additional 6-point subsidy

Treatment province	County	Control province	County
Uşak	Sivaslı, Karahallı, Ulubey, Eşme	Denizli	Çivril, Bekeilli, Çal, Güney
		Manisa	Alaşehir, Sarıgöl, Kula, Selendi
Kütahya	Pazarlar, Simav, Tavşanlı, Domaniç, Şaphane	Manisa	Selendi, Demirci
		Bursa	Keles, İnegöl, Harmancık
		Bilecik	Bozüyük
Afyonkarahisar	İhsaniye, İscehisar, Bayat, Emirdağ, Çay, Sultandağı, Şuhut, Dinar, Başmakçı, Evciler	Eskişehir	Seyitgazi, Han, Çifteler, Sivrihisar
		Konya	Çeltik, Yunak, Akşehir, Tuzlukçu
		Isparta	Yalvaç, Senirkent, Uluborlu, Keçiborlu, Yeşilova
		Denizli	Bozkurt, Çardak, Baklan, Çivril
Bartın-Kastamonu	Ulus, Pınarbaşı, Daday, Araç	Karabük	Yenice, Ovacık, Safranbolu, Eflani
Düzce	Akçakoca, Cumayeri, Gümüşova, Gölyaka, Kaynaşlı, Yığılca	Zonguldak	Alaplı
		Sakarya	Kocaali, Hendek
		Bolu	Mudurnu, Mengen
Kırşehir-Kırıkkale	Yahşihan, Bahşılı, Karakeçili, Çelebi, Sulakyurt	Ankara	Kalecik, Elmadağ, Bala, Evren, Şereflikoçhisar

Notes: The table lists the treatment counties that share a border with a control county, together with the contiguous control counties in each case. The sample comprises 34 treatment counties in 8 provinces and 43 distinct control counties in 12 provinces. Çivril (Denizli) and Selendi (Manisa) each adjoin treatment counties in two different treatment provinces and therefore appear twice in the table; they enter more than one county pair in the estimation.